THE BOSTON
MEDICAL AND SURGICAL JOURNAL.

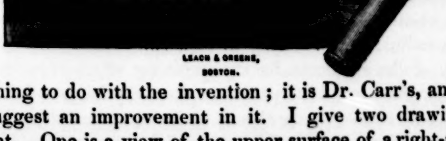
VOL. XCV. — THURSDAY, AUGUST 17, 1876. — NO. 7.

COLLES'S FRACTURE AND DR. CARR'S SPLINT.

BY HENRY A. MARTIN, M. D.,
Brevet Lieutenant-Colonel and late Surgeon U. S. V.

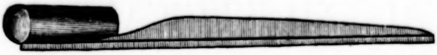
Fractures of the lower inch or two of the radius constitute by far the most frequent of this class of lesions. Even the clavicle is not so frequently reported as fractured, although every case of broken clavicle is known at once, while very many fractures of the lower inch of the radius escape vulgar and even professional detection, and are classed as bad "sprains" merely. It is only when too late for remedy, when swelling has subsided and fragments united, that the characteristic deformity reveals, with certainty, that the case has been one of fracture. I need not state here how numerous have been the forms of apparatus contrived to meet the special exigencies of treatment of these cases. You can find in Hamilton's excellent book over thirty pages devoted to Colles's fracture, and delineations and descriptions of a great many methods for its treatment, including one of Dr. Hamilton's own invention. Within a few months quite a book has been published in London, the principal topic of which is the treatment of Colles's fracture by an improved splint, invented by Professor Gordon, of Queen's College, Ireland. Professor Gordon has won great fame by the splint of which he now offers an improved form, and I think justly, for his splint is, I believe, the only one except Dr. Carr's which recognizes at all a principle of which I consider the splint I am about to describe to be a far more perfect recognition. Dr. Carr is a physician practicing in Goffstown, N. H., whom I have never seen, and with whom my only acquaintance is by report and correspondence. I mention this fact, and also another, that I have nothing to do with the invention; it is Dr. Carr's, and his only. I cannot suggest an improvement in it. I give two drawings of Dr. Carr's splint. One is a view of the upper surface of a right-arm splint,

No. 1.



the other a profile of its radial side. Its length is eleven and one half inches, its width two inches. It consists of a strip of light wood one

No. 2.



sixth of an inch in thickness, on which is laid a carved wooden "bed" of peculiar form, the irregularly convex surface of which is exactly adapted to the concavity of the lower side of the radius. An oblique cross-piece or "bar," round, four inches long and one inch in diameter, is attached to its distal end. The original peculiarity of the instrument is that its author was the first who has, apparently, reflected that the radius is a much-curved bone, and that treatment of its fractures on a perfectly flat splint can only result in more or less deformity in the bone itself, and in such a change in the relations of its lower articulating surfaces as must, almost inevitably, induce greater or less impairment of the symmetry, usefulness, and comfort of the wrist. In Professor Gordon's splint, the concavity of the radius is recognized as an indication for treatment; but in even the improved form of that apparatus the convex "bed" is applied to the *side* of the bone and not *under* it. For American surgery I must claim that Dr. Carr's splint was invented in 1843, long before that of Professor Gordon. The cross-bar is to support the hand; when the latter grasps, or is firmly bound to it, it will be perceived that the hand is flexed and inclined towards the ulnar side of the arm, and the constantly observed tendency to subluxation of the ulna and very prominent and perceptible deformity resulting therefrom is obviated. There is nothing original in this cross-bar; it is to be found in Bond's very ingenious splint, and in others.

When a patient comes to me with a Colles's fracture, I take the splint proper to the side injured, lay along its upper surface a quadruple fold of old cotton sheeting, a little wider than the whole width of the splint, and extending from the cross-bar to an inch beyond its proximal extremity. The lower surface of the fore-arm is then laid on this, so that the cross-bar comes very exactly under the metacarpo-phalangeal articulations. Two or three turns of bandage may now be thrown around the middle of the fore-arm, binding it firmly to the splint, and the patient requested to grasp the cross-bar as strongly as may be; in this he may require the aid of the surgeon's hand. This action of the hand will reduce at once a very large proportion of all fractures of the lower part of the radius, and its doing so is a sufficient proof of the perfect adaptability of the apparatus for the lesion for which it is designed. Of course, the surgeon can, if he prefers, reduce the fracture *before* applying the splint; some cases require this to be done, and *all* those in which the fracture is complicated with subluxation of the two rows of carpal bones, which should be looked for sharply, and not placed in the splint till *thoroughly* put in proper position. A properly padded, very light, thin splint strip, say eight or nine inches long and two inches wide, may now be laid along the back of the radial side of the fore-arm from the carpo-metacarpal junction. This dorsal splint is not always absolutely necessary, but may as well be always applied. All that is now requisite is to bandage in the usual way; the bandage may extend upwards from the middle of the metacarpus to the proximal end of the splint, leaving — and this is *very* important — the fingers to be exercised and even used freely, for holding fork, spoon, etc. After the first week the bandage need extend no farther than to the carpo-metacarpal articulations, leaving the patient free and useful motion of the hand. If the bandage, however, is thus limited, the surgeon should see that the carpus is firmly and immovably attached to the splint; this may be well done by winding a strip of good sticking-plaster, an inch and a quarter wide, three times round the wrist and splint. The great value of this mode of treatment is that it relieves the patient of much discomfort and enforced disability, and that, when the fracture is sufficiently consolidated for the splint to be removed, the hand will be ready for use, and entirely free from the stiffness, often amounting almost to anchylosis, so familiar to hospital surgeons and all who have had or seen much of this sort of surgery, — a stiffness rendering the hand more or less useless for weeks, months, and sometimes even years. In fifty cases of "simple fracture near the lower end of the radius, only sixteen are positively known to have left no perceptible deformity or stiffness about the joint." "Twice I have found the wrist and finger joints quite stiff after the lapse of one year; in one case I have found the same condition after two years, in one case after three years, and in two cases after five years." "If we confine our remarks to Colles's fracture, the deformity which has been observed most often, and, indeed, with only rare exceptions, *being found in some degree, more or less, in several of those cases which I have marked as perfect*, consists in a projection of the lower end of the ulna inwards and generally a little forwards."[1] In the part of Dr. Hamilton's excellent work from which these extracts are made may be found a great deal more in regard to the frequent and persistent stiffness, deformity, and lameness consequent upon these fractures. Dr. Hamilton combats the doctrine that the stiffness of the finger-joints is a result of extension of inflammation to those joints, and maintains, what is doubtless true to a certain extent, that it results from an "effusion, first serous and subsequently fibrinous, along the sheaths of the tendons;" "an anchylosis produced, not as has generally been affirmed, by extension of the inflammation to these [the finger] joints, but by the inflammatory effusion and consequent adhesions along the thecæ and serous sheaths."

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[1] Hamilton on Fractures and Dislocations, Philadelphia, 1860, page 279. The italics are mine. — H. A. M.

"The fingers are quite as often thus anchylosed as the wrist-joint itself, a circumstance which is wholly inexplicable on the doctrine that the anchylosis is due to an inflammation *in* the joints." Dr. Hamilton[1] says that the same stiffness, etc., is as often to be noted after severe sprains and other severe injuries of the wrist as in fractures, and infers that the more or less perfect adjustment of the fracture has nothing to do with these disagreeable *sequelæ*. I should say that when a fracture is imperfectly reduced, the degree of inflammation at and about the seat of injury must be great and the amount of effusion large. When the fracture is without delay and *perfectly* "set," both would be reduced to a minimum.

With this splint applied, the patient can with ease and safety wear his ordinary coat, and take it off and put it on with little or no assistance. The length of time before the splint may be safely removed must depend a great deal, of course, on the prudence and occupation of the patient.

In the treatment I now advocate there is a most perfect coaptation, and, as I have thoroughly ascertained by very many observations, *no tendency whatever to displacement* so long as the bandaging is kept properly close. In the last case (just discharged) under my care, a laboring man sixty-three years old, I removed the splint on the eighteenth day, found the wrist in perfect "drawing," told him to wear a sling, and come to see me every three days. I saw him at these intervals for two weeks, during which there was not the slightest tendency to distortion. In the first dozen or fifteen cases I treated with this apparatus I saw the patient five or six times in the course of treatment, and almost every time unwound the bandage, keeping the splint carefully in place; in not one instance did I find the slightest tendency to displacement. This was, of course, experimental, to ascertain the capabilities of the method of treatment. I by no means recommend meddling with and frequent examination of fractures, as when I narrated the above experiments I was most unkindly and disingenuously twitted with doing by a famous New York surgical specialist, at the late meeting of the American Medical Association at Philadelphia. In treating Colles's fracture, now, I never take off the bandage nor even partially unwind it, unless to tighten it, from its first application till its final removal, nor would I require to see the patient in the intervening time but for the probability of loosening, or the possibility of slipping of the bandage or splint.

I have already, in six years, treated about forty-five cases, of all degrees of severity and complication, in which the lower two inches of the radius was the seat of fracture. In none of these was deformity to be recognized at the time that the apparatus was removed. I have seen many of these cases years after the injury, and in not one have I been able to detect which side was injured, from any abnormal variation.

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[1] Pages 279, 280.

A most absolute proof of the perfect adjustment attained by Carr's method is found in the entire relief to the pain very soon after the arm is "put up." I have given Leach and Greene, instrument makers of this city, approved patterns of Carr's splint, only stipulating, as there is no patent, reserved right, or royalty of any sort to be paid, that they will afford it to the profession at as low a price as may be consistent with a reasonable profit.

I have omitted to recommend the use of Carr's splint in cases of sprain of the wrist. In no other way that I am acquainted with can such perfect rest and consequent immunity from pain be procured for such cases. There is a large class of cases of injury of the wrist, in which fracture of the styloid process of the radius exists, but cannot be made out with absolute certainty; when such cases are treated as sprains merely, the positive fact of fracture is ascertained too late for remedy. It is not always often easy in *any* case of so-called sprain of the wrist to say, positively, that fracture does *not* exist, and even where there is none of the bone there is always contusion, twisting, and partial or complete fracture of interosseous ligaments, injuries requiring rest as much as if the bone were broken.

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THE USE OF AROMATIC SULPHURIC ACID IN NECROSIS.

BY EPHRAIM CUTTER, M. D., CAMBRIDGE.

April 10, 1875, Dr. A., of Worcester, requested the writer to remove the necrosed alveolar process of his wife's sister. She was of middle age, pale, thin, weak, anxious, and worn. She had suffered much with her teeth. Her upper right middle and two lateral incisors were found to be loose, and their lower edges hanging below the line of their fellows. There was a fungoid, spongy swelling over the front of the diseased process. When this was pressed, pus freely exuded from several openings, and also from a softish, elastic swelling as large as a hazel-nut, situated at the dome of the hard palate inside the mouth. The loosened teeth could be freely moved in every direction with the thumb and fingers. The roots of the teeth distinctly grated against the sound alveolar process. There was a complete separation of the teeth and the bone. Dr. A. said that he had thought of using the aromatic sulphuric acid, but that the disease was so extensive and the separation so complete that he regarded it as useless to try to save the teeth in any way. It appeared to the consulter, however, while the surgical extirpation would be effective and justifiable, that if free incisions were made into the swollen and spongy gums there would be an evacuation of the contents of the dilated capillaries and abscesses; that a healthy action would be promoted by relieving this unnatural distention, and that the necrosed bone might be slowly removed by the stimulation of the aromatic sulphuric acid topically applied without destroying the teeth. It was thought that then the periosteum would lay down new bone in place of the old, and refasten the teeth in their old place. It was agreed to employ the following: —

℞	Aromatic sulphuric acid	ʒj.
	Aquæ	ʒj.

By means of a half-ounce syringe supplied with a small ivory tip, one inch and a half long, and one eighth inch in diameter, the acid solution was injected at first twice a day and afterward once a day. About two drachms were used at each injection. The syringe tip was deeply buried into the soft tissues through one of the openings. Pus would freely exude from the other openings, even from that in the top of the mouth after each injection.

Tonics were administered. A diet of animal food and unbolted wheat was rigidly maintained.

From the outset of this departure a marked improvement in the soft tissues occurred. But the teeth remained loose and dangling, and Dr. A. thought their recovery doubtful. It was re-suggested that it would be an easy thing to remove them at any time if they did not reset, but that the process of replacing old with new bone was of necessity a slow one.

In about forty days the outer incisor became solidly fixed in its old site. Then the next incisor also tightened. The middle incisor tightened slowly. In November following it could be very slightly moved, but its edge was a little below the line of the other teeth. The other two incisors were as stiff as they ever were. A few spiculæ of bone were removed from the front of the alveolar process during the period of the treatment. In the mean time the general health of the patient improved greatly. She gained in weight, color, and strength. At the present time (July, 1876) she is entirely recovered.

We think it is reasonable to connect the result in this case with the means employed, the acid, the tonics, and the food.

Dr. Atkinson, of New York, has reported some remarkable instances of cure of necrosis by this agent used in its full strength, it is said. It hastens the disintegrating and separating processes, and at the same time destroys the germs of parasitic micrographic growths in the dead and dying bone. According to Dr. Atkinson, it does not act unhealthily upon sound tissues whose vital connections are unimpaired. No substances stand higher than the mineral acids as antiseptics and destroyers of bacteria, amœbæ, and vegetations of animal secretions. Were it not for their caustic effects they would long ago have supplanted carbolic acid.

RECENT PROGRESS IN OBSTETRICS AND GYNÆCOLOGY.

BY W. L. RICHARDSON, M. D.

OBSTETRICS.

Ligation of the Umbilical Cord. — In a paper recently read before the Société de Biologie of Paris,[1] Dr. Budin gave the results of two series of experiments which he had recently performed. Each series comprised a number of carefully noted observations. In the first set, of thirty-two cases, he waited, before cutting the umbilical cord, until the pulsations had ceased; while in the second set, of thirty cases, he cut the cord immediately after the birth of the child. In both series of cases he preserved all the blood which escaped from the placental end of the cord. He found that about twelve cubic centimetres of blood were lost in those cases in which the ligation of the cord was delayed until after the pulsations had ceased, while nearly one hundred cubic centimetres were lost when the cord was cut while yet pulsating. From these experiments Dr. Budin concludes that the cord should never be cut until all pulsations have ceased, as otherwise the child loses eighty-eight cubic centimetres (3.08 oz.) of blood. The accuracy of these experiments is recognized when it is borne in mind that the fœto-placental circulation is a closed circuit, and that therefore no blood can escape from the placental tissue externally except through the cut end of the umbilical cord. By waiting until the pulsations of the funis have ceased, a large amount of blood is saved to the fœtus which must otherwise be lost. The time during which the cord pulsated after the birth of the child averaged two and a quarter minutes, the longest time being fifteen minutes. By allowing the blood thus to be withdrawn from the placenta, its subsequent expulsion is greatly facilitated, owing to its diminution in size.

Rupture of the Uterus.[2] — Dr. Bandl gives an account of thirty-two cases of labor in which this accident occurred. In no case did he find the pathological change in the uterine substance which has generally been considered as the predisposing cause, especially in multiparæ. The uterus was always thick, well contracted, and the cervix very thin. In nearly every case the fissure was found to be in the cervix, and even in those cases in which the body of the uterus was torn, the rent began in the cervix. The peritoneum was never separated from the fundus. Dr. Bandl believes that the rupture is always due to some disproportion, since he found that, in nineteen out of the thirty-two cases there was a narrow pelvis, in three hydrocephalus, in eight a shoulder presentation, in one a prolapse of the foot with the head, and only one case was observed which could not be explained. Pressure does not produce a rupture, as frequently sloughing occurred without rupture, and in one case both were observed in the same patient, though situated at different points. An unyielding os uteri, rudimentary or double development, and fibroid tumors are often stated as causes, but this is not proved. Bandl agrees with Chiari, Braun, and Späth in considering that the abnormality is due to an excessive thinning of the cervix occurring during labor; he has found by actual measurement that the walls of the uterus in multiparæ, especially when there has been a disproportion in previous labors, were much developed. In normal circumstances the cervix is drawn back over the head of the child by the muscular uterus, the orificium internum remaining, as investigations on the living and dissections on the dead subject show, about the level of the pelvic brim. If there is a disproportion which prevents the presenting part from descending into the pelvis, the cervix is abnormally stretched, the internal orifice is raised a hand's breadth above the brim, and a rupture becomes possible. If this abnormal condition has once existed it takes place more easily in future labors, and thus we find ruptures occurring more commonly among multiparæ. Dr. Bandl believes that a rupture can be recognized as threatening when the internal orifice gradually ascends whilst the cervix stretches and the fundus acquires a lateral position.

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[1] Bull. Gen. de Thérap., February, 1876.

[2] Brit. and For. Med. Chir. Rev., April, 1876, from Centralblatt, No. 33, 1875.

The Relation between the Sex of the Fœtus and the Fœtal Pulse.[1] — Dr. Mattei reports the result of a careful study of several hundreds of cases. The examination was made before the labor began, and when the child was undisturbed. He bases his opinion on the rule that if the pulse-rate is below 135, the fœtus is a male; if above 150, it is a female. In only three cases did his prediction turn out to be wrong, and in all of these after he had predicted males the fœtuses were found to be small and feeble females, two of which soon showed symptoms of congenital syphilis. In these three cases, therefore, he considers that the slow pulse-rate was due to the feeble condition of the fœtus.

Separation of the Symphysis Pubis during Labor.[2] — The extent of separation which can take place between the two pubic bones is a subject which has given rise to much discussion among obstetricians. Some writers have gone so far as to maintain that a limited separation takes place in all cases of labor, while others have as positively asserted that no separation is possible, without giving rise to serious results to the mother.

In the obstetrical clinic at Erlangen, a patient thirty years of age was confined with her fourth child. She was a woman of medium size, healthy, and her previous labors had been perfectly normal. Her fourth labor progressed naturally until after the escape of the liquor amnii, when it became very tedious. The child presented in the dorso-posterior position of the right shoulder. By bi-manual version this position was changed to the second position of the breech. The os slowly dilated, and as soon as the hand of the attendant could be introduced, version was performed and the child delivered in an asphyxiated condition. A few days later the patient began to complain of pain referred to the symphysis pubis. This pain was aggravated by the slightest movement of the lower extremities. An examination detected a complete separation of the two pubic bones. A belt was applied by means of which the two surfaces of the bones were brought together. At the end of a month the patient was discharged, a firm union having taken place between the bones.

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[1] Archives de Tocologie, March, 1876.

[2] Revue de Thérap. 2, 1876.

Pregnancy with Unruptured Hymen.[1] — Dr. Gustav Braun gives an account of three cases in which he attended women in their confinement, the hymen at the time being unruptured. As a result of a careful examination of these cases, Professor Braun concludes that an unruptured condition of the hymen is not of itself a positive sign of virginity. The hymen may be so elastic as to allow the penetration of the penis without leaving any trace of its presence in the vagina. Two of the cases afforded conclusive evidence that the deposition of semen within the vulva was all that was required to produce impregnation, since in those cases there had been no penetration within the vagina. One of the cases showed the advantage which a spontaneous opening in the hymen for the escaped of retained menstrual fluid has over an artificial one. An imperforate hymen is no prevention to delivery, the prognosis in these cases being favorable. One of the cases showed how the urethra may take the place of the vagina for the purposes of copulation without any subsequent disturbance of the function of the bladder.

(*To be concluded.*)

PROCEEDINGS OF THE OBSTETRICAL SOCIETY OF BOSTON.

C. W. SWAN, M. D., SECRETARY.

THE society met at the house of Dr. Curtis, 2 Spruce Street, February 12, 1876, at 7½ P. M. The president, DR. HODGDON, in the chair.

Complete Absence of the Menstrual Function. — DR. MINOT said that he had recently been consulted by a single lady, thirty-four years old, in reference to a slight indigestion, and learned that she had never menstruated, nor had she ever had any symptom referable to an attempt at menstruation, such as pain, discomfort, fullness, oppression — in short, any symptom which could be referred either to the presence or the absence of the catamenia. So far as could be judged from external appearances she was a fully developed woman in perfect health.

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[1] Wiener Med. Wochen., March 25 and April 1, 1876.

Uterus Subseptus. — **Dr. Richardson** reported the case. The uterus had two ora, the rest of the uterus and the vagina being normal. The case is published in the Journal, xciv. 349.

Dr. Chadwick expressed a doubt whether this case might not be, after all, that of a double uterus. Küssmaul, he believed, made no mention of so restricted a duplicity as this in his exhaustive treatise.[1] The Italian case cited by Dr. Richardson seemed to be the only exception to the general rule that, in the development of the genito-urinary tract from the fusion of the two Müller's ducts, at whatever point the fusion was arrested, the whole canal above that point remained double. The fact that the canal of what seemed to be a supernumerary cervix opened into the other canal could easily be explained by supposing that, owing to some laceration during labor, a fistula had formed in the septum dividing the two; a second uterine cavity might, on this supposition, exist, although the sound had not been made to enter it.

Dr. Richardson replied that the fusion of Müller's ducts normally begins in the middle. Schroeder alludes to two or three cases which are variance with Küssmaul's statement as quoted by Dr. Chadwick.

Dr. Forster called attention to the possibility of error in diagnosis as having actually occurred shortly before one of the confinements of the patient described by Dr. Richardson. The physician, ignorant of the peculiarity, and finding by digital examination the lateral and unused os without coming upon the other, failed to recognize the stage of labor present.

Incarcerated Pessary. — **Dr. Lyman** reported the case of a lady who, married ten years, six years ago had her last child, followed, apparently, by some prolapse or retroversion of the uterus. She went abroad, and in Leipzic consulted Crede, who found slight retroversion, and introduced a pessary. There it remained till, recently, five years later, Dr. Lyman removed it. The extraction was difficult, and attended with pain and slight hæmorrhage. The upper half of the pessary had become roughened and eroded, and was completely imbedded in the posterior cul-de-sac. The lower half was sound, but corrugated. The material was thick soft-rubber tubing covering a wire which was bent into the shape of a Hodge's closed pessary. The cervix uteri was deeply ulcerated, and there was considerable leucorrhœa. There had been no interference with the action of the bowels, but the patient had had a constant sense of something to come away, whenever she was in the erect position. Dr. Lyman exhibited a rubber pessary, in the possession of Dr. Chadwick, which is said to have been worn for seven years. It was much eroded and roughened.

Dr. Buckingham stated that he had not introduced a pessary for several years; he had generally found them out of place when long worn. One, which had been worn two or three years, and had caused a good deal of backache and leucorrhœa, he had found tipped up edgewise in the lower part of the vagina.

Dr. Lyman said that though pessaries were often needed and useful they were often introduced without necessity, and that he had taken away more than he had put in.

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[1] Von dem Mangel, der Verkümmerung, und Verdopplung des Gebärmutter. Adolph Küssmaul. Würzburg. 1859.

Dr. Chadwick stated that he had also taken out almost as many as he had introduced during the past three years; many of the pessaries removed had given rise to acute inflammation of the uterus or surrounding tissues; all had failed to effect the object for which they were designed; though believing them to be of great service in the treatment of certain conditions, he deprecated their promiscuous use to correct any and every slight displacement of the uterus.

He was reminded by Dr. Richardson's case of another anomalous condition occasionally found in the vagina, namely, the union of the anterior and posterior walls, as a result of acute inflammation, so as either to occlude the canal completely, or, what was more common, to produce partial atresia. He had several times seen the resulting deformity, but had never witnessed the process itself until recently. A widow, fifty years of age, had applied at his dispensary for treatment of a very intense inflammation of the whole vaginal canal with profuse purulent discharge; the disease was unquestionably of gonorrhœal origin, and was of two years' duration. She had ceased to menstruate eight years before. After three or four visits Dr. Chadwick had been one day amazed to find the vagina occluded an inch and a half above the vulva; he was, however, able to dissect the walls apart by his finger, and thus reëstablish the canal in its full integrity. Through the speculum the mucous membrane of the apposed walls could be seen to be denuded in some places and in others covered with shreds of false membrane. This dissection had to be repeated several times, but the woman has since been discharged, well.

Dyspareunia. — **Dr. Wellington** related the case of a patient whom he had been engaged to attend in labor. She had consulted a physician in the city in reference to intense suffering during the whole sexual act, dating from her marriage, two years before, and was told that there was no apparent reason for her difficulty, which would probably disappear after labor. The labor was severe, and was terminated by forceps. The perinæum was ruptured, was sewed up, and healed well. A day or two ago she was very well, but the pain of intercourse was as bad as ever. The finger in the vagina gave no pain, and the parts appeared to be perfectly normal. Dr. Wellington asked for the experience of other members of the society in this matter.

Dr. Lyman gave the case of a young married lady, perfectly well formed, with large hips, who suffered intense pain — vaginismus — during sexual intercourse. On examination it was found that the hymen and never been fully ruptured. A fenestrated, hard-rubber tube was introduced with some pain, and the patient was instructed to repeat the application. There was only temporary relief from this means. A second examination disclosed an herpetic patch inside the inner labia, which was the source of the trouble. The vaginal orifice was powerfully stretched with the two fingers, the herpetic patch was cauterized with the nitrate of silver, and a pledget of lint wet with lead-water was kept applied to the cauterized region. The parts healed entirely, the symptom disappeared, and the patient will be confined in a few months.

Dr. Minot remarked that such cases are by no means rare. He had seen several. One was like that reported by Dr. Wellington. The patient was delivered of a blighted fœtus, but no relief followed. She was then etherized, and the sphincter vaginæ was ruptured. She afterwards had a child at full term, but the hyperæsthesia continued exactly as before. In New York she underwent some operation at the hands of a gynæcologist without material benefit then or since. Another case was that of a woman sixty years of age who had suffered during the whole of her married life from vaginismus. There is every variety of the disease and every degree of suffering. Some patients have simply no pleasure in the sexual act; others experience disgust, and so on to the extremest suffering. Many of the cases may be regarded as neuroses, in the absence of other assignable cause, and occur in women of marked nervous temperament. This was the case with the patient first mentioned.

Dr. Buckingham briefly referred to some of the more obvious causes of the malady, such as the disproportion in size or length of the relative organs, and not infrequently the occurrence of painful vascular tumors at the orifice of the urethra.

Dr. Chadwick mentioned as among the causes the occurrence of hypertrophied papillæ in the vaginal entrance. He stated that he had seen but one case where the pain could not be referred to this cause, while, on the other hand, these growths are not always tender. They are best seen by separating well the thighs, and are found standing up like rows and groups of needles, one sixteenth to one eighth of an inch in height, in the groove between the urethra and the clitoris, on either side of the labia minora and at the posterior commissure. They may be snipped off and the bases cauterized with nitric acid, but they are extremely difficult to cure. In one or two cases of painful intercourse he had found an injected, eroded spot at the base of the hymen.

Dr. Forster stated that he had had a case in which he found mucous patches. These cured, there was no further trouble.

Dr. Abbot said that he thought, with Dr. Minot, that many of these cases are neuroses. He had found the condition to be not always uniform, but varying considerably with the nervous state of the individual, sometimes depending upon a disproportion of desire, or an excess of it, on the part of the husband, or a too romantic and sentimental idea of marriage on the part of the wife, as in a case cited in illustration.

Dr. Minot stated, in answer to a question, that he had seen but one case in which painful intercourse did not begin from the first.

Dr. Abbot mentioned the case of a woman whom he attended in her first confinement, after being married six years. He saw her at the beginning of labor, when the tenderness of the vagina was so exquisite that he was obliged to etherize her in order to make an examination. The tenderness had existed during the whole of her married life.

Blighted Ovum. — **Dr. Buckingham** showed the specimen, which occurred in the practice of Dr. J. W. Cushing. It was a case of twin pregnancy and single labor, the blighted fœtus coming away with the placenta after the birth of a healthy child at term. The case will be fully described hereafter.

A Rare Form of Menstrual Blood. — **Dr. Lyman** showed the specimen. The patient is a woman fifty years old, single, and never having had connection. She menstruates monthly, and loses each time a large amount of blood. Once in two or three months she throws off from two to four ounces of semisolid material, which when fresh and infiltrated with fluid might upon superficial examination be mistaken for hydatids, but which on standing is seen as soft, brown, fringed flakes of blood. It has been examined, and pronounced to be simply blood. The case will be reported more fully after further observation, the patient being seen to-day for the first time.

Caustics. — **Dr. Lyman** inquired of the gentlemen present their experience in the use of an alcoholic solution of bromine upon the cervix uteri, and what advantage it possesses over glacial acetic acid or any other caustic. He preferred and had used for the past twenty-five years the acid nitrate of mercury in all cases requiring anything more than superficial action. He had never seen salivation result, but he had always used it with caution, never smearing it over the whole cervix, and carefully limiting its action with the application of chalk.

Dr. Richardson stated that, at Dublin, he had twice seen patients badly salivated by the application of the acid nitrate of mercury to a greater or less extent over the whole cervix.

Multiple Spontaneous Abortions. — **Dr. Chadwick** reported the case. The patient is a woman twenty-eight years old, who has miscarried at from two and a half to three months — never exceeding that time — ten times in the past five years. She applied to be relieved of the habit, and stated that she had never done anything to induce abortion. The accompanying pain and hæmorrhage had been slight. A little trickling of blood would be the first indication, and in a week or ten days the ovum would come away. Upon examination the vagina was found to be small, and the uterus, though of nearly normal length, exceedingly flabby and difficult to define. Otherwise, nothing abnormal was discovered. Neither the woman nor her husband had had syphilis, and the latter was moderate in his sexual appetite. More recently the patient presented herself when six weeks pregnant. The body of the uterus was found to be unusually soft, even for the impregnated condition. She was advised to take to her bed at two and a half months and remain there perfectly quiet, at the same time abstaining from all sexual intercourse. These directions were implicitly followed, but at the end of the third month the trickling of blood began, and she aborted as before. The ovum was subjected to very careful examination, but it appeared to be perfectly healthy. The patient recovered well, but on reëxamination the same condition of the uterus was found. The walls of the cervix were very thin. Dr. Chadwick stated that he was now trying the effect of Simpson's galvanic stem, which had been difficult of insertion on account of the flabbiness of the uterus, but was borne by the patient without the slightest sensation.

Dr. Buckingham suggested the use of ergot in large doses, say drachm doses, several times in a day, until the pulse should be brought down below sixty in the minute. The object of this would be to get the alterative effect upon the uterus.

LECTURES ON FEVER.[1]

We have received from Henry C. Lea, of Philadelphia, a volume of some two hundred and fifty-odd pages, entitled Lectures on Fever, by William Stokes, M. D., D. C. L. Oxon., F. R. S., and edited by John William Moore, M. D., Assistant Physician to the Cork Street Fever Hospital, etc.

This book is one of the most valuable contributions to the literature of clinical medicine which has appeared for some time. It is always a matter for congratulation when men of large experience find time to publish the records of their observations; and this is especially the case with regard to Dr. Stokes, whose well-known powers of accurate observation and reasoning make all his communications worthy of our most careful attention. The volume under notice contains a series of thirty-three lectures on fever, delivered in the theatre of the Meath Hospital and County of Dublin Infirmary. Their delivery was spread over a considerable period of time, and were not given in any regular sequence so as to form a continued or systematic course. Most of them were delivered at irregular intervals, and all as extemporary discourses. Some of the lectures were delivered as far back as the year 1854, and appeared in the *Medical Times and Gazette* under the head of Clinical Lectures on Fever.

The most interesting lectures in the series are the twelfth to seventeenth, inclusive, on Secondary Pneumonic Complications, The Heart in Fever, and the twenty-sixth, twenty-seventh, and twenty-eighth, on Treatment and the Use of Stimulants in Fever; and finally, Lecture XXIII., on the complication not infrequently met with, phlegmasia alba dolens.

In the earlier lectures Dr. Stokes has considered in some measure the question of the separate identity of typhus and typhoid fevers. He has preferred, and we think wisely, to dwell on the great subject of the relation of the secondary affections of fever to the essential malady, and in the light of that relation to discuss the question of treatment.

In Lectures XII. and XIII., the author very ably discusses the subject of secondary pneumonic complications of fever. In speaking of the term typhoid-pneumonia, he says: "You will be convinced when your experience has been enlarged, that under this term many different forms of disease have been classed, and it is very doubtful whether a true pneumonia is ever developed in the course of a fever. You will meet with the physical signs which attend pneumonia; but these, as you all must know, are insufficient to establish the existence of the disease, and even these very physical signs are seldom so well marked, so complete, as it were, as in simple inflammation of the lung. Nor again do they follow in the regular succession which we find in true pneumonia." A little farther on he says: "Here let me warn you against a common error with respect to cases of disease of the lung arising in the course of some form of constitutional malady or fever. They are usually set down as pneumonia, — typhoid-pneumonia by some. Now, the name itself would be of little moment if its adoption did not lead to errors in practice; and although it cannot be affirmed with certainty that in none of these cases is there pneumonia, yet we have good grounds for believing that in many of them inflammation, as the term is commonly understood, is either absent from the first, or, if it occurs, that it is only secondary to a special lesion induced by some form of essential disease. It is difficult to give any well-defined classification of the various forms of diseases described under the head of typhoid-pneumonia, or to draw the line between simple asthenic inflammation of the lungs and those conditions described from an early period under the terms of bilious, putrid, or typhoid-pneumonia. And observe that when I make use of the term asthenic-pneumonia, I refer more to the condition of the general system than to the activity or inactivity of the local disease. For so far as local inflammatory action is concerned, there is proof that it may originate and proceed with rapidity, and even with vehemence, in the very last periods of life so that the disease may be sthenic quoad the local condition, and yet the case be itself asthenic in reference to the general state of the economy. Much of the confusion with regard to this subject has arisen from the circumstance that too great weight was attached to the presence of certain physical signs which were taken as always indicating similar vital conditions."

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[1] *Lectures on Fever.* Delivered in the Theatre of the Meath Hospital and County of Dublin Infirmary. By William Stokes, M. D., D. C. L. Oxon., F. R. S., Regius Professor of Physic in the University of Dublin; Physician to the Queen in Ireland. Edited by John William Moore, M. D., F. K. Q. C. P., Assistant Physician to the Cork Street Fever Hospital, etc. Philadelphia: Henry C. Lea. 1876.

In Lecture XIV., entitled The Heart in Fever, there is considerable interesting matter. "Long ago," says Dr. Stokes, "Laennec stated that in certain cases of low fever he found the heart in a softened condition, in what he termed l'étât poisseux of the muscles of that organ. He suggested that in those cases which exhibit rapidity of pulse during convalescence this symptom is to be attributed to a softening of the heart. Dr. Stokes does not consider this opinion well founded; he thinks that the true typhus softening of the heart, so far from being followed by rapidity of the convalescence, has much more frequently the effect of making it slow. Slow not only as considered with reference to the condition of health, but actually falling below the ordinary standard. That rapidity of pulse is commonly associated with typhus affections of the heart, in which there is weakening of the organs while the fever continues, must be admitted. But this is a different proposition from that advanced by Laennec, who speaks of the symptoms during convalescence."

A little farther on Dr. Stokes, in speaking of the significance of rapid pulse, says: "You may rest assured that rapidity of pulse in convalescence, so far from indicating any remains of the typhus disease in the muscular structure of the heart, is in most cases a proof of the existence of some lurking disease of some important organ or organs. The local diseases which most frequently attend this condition are of two kinds: one of them tuberculosis of the lungs and other parts; the other is the existence of a secondary reactive inflammation in the mucous glands of the intestine."

Dr. Stokes gives special warning in regard to the complication "Phlegmasia alba dolens," which may occur without any distinct notice of the invasion of the disease.

In Lecture XV., he states that he considers exemption of the heart from typhus affection a ground for a favorable prognosis, while continued excitement of the heart is equally a ground for a bad prognosis. In speaking of cases of continued excitement of the heart, he says: "This condition of the heart, whether it has existed all through, whether it is intercurrent or ephemeral, or, as we say, terminal, coming on at the close of the case, or lasting many days, is, I need not tell you, of the worst augury."

In Lecture XVI., he speaks of depression of the heart being more marked in typhus than in typhoid, of the impulse of the heart, and closes with the following remarks: "I have mentioned that the sounds of the heart become altered in the course of a fever: the change begins in the left ventricle, and travels towards the right side of the heart; the systolic sound first becomes diminished towards the left, and afterward towards the right. In cases of recovery the phenomena follow the inverse course, as we might naturally expect. We find the returning first sound audible, first over the right and then over the left ventricle.

Lecture XVII. The diagnosis of actual softening, as distinguished from simple debility, will depend on the character of the fever and the time during which the physical signs of failure of the heart have been present. Experience again shows that the progressive diminution of the impulse and sounds of the heart, which may proceed to their extinction, is sufficiently often connected with the actual softening to justify the diagnosis of this lesion.

And again, referring to prognosis, he says: "It is clearly to be more favorable in the case of depression than in that of excitement of the heart; and in looking at the frequency of softening, we arrive at the conclusion, which seems a strange one, that the existence of such a change in one of the most important organs of the body may lead to a better prognosis than under opposite circumstances. The reason of this is obvious. In the case of typhus softening we can safely and advantageously employ stimulants which are comparatively ineffective, and often inadmissible when the heart is excited."

The lectures on treatment are less interesting than the others, with the exception of that portion where the subject of stimulants and feeding is discussed. The precise indications for the administration of stimulants will bring joy to the heart of the young and inexperienced practitioner.

Dr. Stokes criticises Dr. Todd in a kindly manner, in regard to stimulants, and agrees with Graves, from whose ninth lecture he quotes in regard to feeding. It was Dr. Stokes to whom Graves made the well-known remark, "Will you, when the time comes, write my epitaph, and let it be, 'He fed fevers.'" After quoting from Graves, he says: "I have now shown you that Dr. Graves was no advocate for the indiscriminate and too liberal employment of food in fever, and that he was fully alive to the dangers both of the want and the excess of food, even in convalescence."

Having previously spoken of anticipative stimulation on page 198, he says: "Take this rule with you into practice, that in the treatment of fever and at almost any period of fever, you are not to be guided by the pulse alone. It must be observed in relation to the action of the heart, remembering that a full and good pulse may coincide with a feebly-acting heart, a heart under the influence of the fever-poison, often, as it were, on its way to the state of softening. All this, you see, bears on the question of the anticipative treatment."

Among the most reliable indications for the early use of this treatment are the physical signs of weakness of the heart, and the following are the physical signs which seem to indicate the anticipative use of stimulants: —

(1.) Early subsidence of the first sound, observed over the left ventricle.

(2.) Diminution of the first sound over the right ventricle.

(3.) The heart acting with a single and that the second sound.

(4.) Both sounds being audible, but their relative intensity being changed, so as to represent the action of the heart of a fœtus in utero.

(5.) With these signs a progressive diminution of impulse, which occasionally becomes imperceptible, even when the patient lies on the left side.

The full administration of stimulants is generally called for when the patient has passed the age of eighteen or twenty. Their energetic use is indicated principally in adults between the age of twenty-five and forty-five; and here we employ them at an early period, not so much to combat existing prostration, but to anticipate the depression of vital power, which sooner or later is almost certain to ensue.

As to quantity to be given of wine, whisky, or brandy, he says: "There are no certain rules; every case has its own peculiarities, even in the same epidemic. You will have differences in the necessity for stimulation, differences in the degree of vital prostration, in local complication, and in all the physical signs of the heart, as to their nature, combination, mode of subsidence, and behavior under treatment."

Lecture XXX. is on the local treatment of secondary affections in fever. The treatment is rather old-fashioned, and the lecture is remarkable as being the least interesting in the whole series.

In Lectures XXXI. and XXXII., the subject of treatment is continued, and one is surprised to find that the hydropathic treatment revived by Brand is not spoken of.

The lectures close with a very interesting one on phlegmasia alba dolens. An interesting case of the complication in question is appended, and as the patient was a physician, and former pupil of Drs. Graves and Stokes, the facts are given in his own words.

In conclusion, we would say of the book that it is written in excellent English, and is characterized by a clearness, comprehensiveness, and modesty, which render it a model of style and method. The publisher has given us clear type, but poor paper. However, we presume the cheap paper lessens the cost of publication, and so places a valuable work within the reach of many who would not otherwise see it. J. P. O.

STATE SOCIETY TRANSACTIONS.

The activity of many of our state societies is made manifest in the annual reports which are now appearing in rapid succession. It is encouraging to notice that the interest in the work of these organizations is becoming more general, and our medical public is beginning to appreciate the great advantages to be derived from developing home resources pertaining to the science and practice of medicine.

We have before us the communications of the Rhode Island Medical Society, which contain a number of original communications in addition to the society reports. The Transactions of the Medical and Chirurgical Faculty of Maryland at its seventy-eight annual session contains a great deal that is interesting. The annual address was delivered by Dr. Roberts Bartholow, of Ohio, on The Degree of Certainty in Therapeutics. The bulk of the society work for the year is put in the form of reports from the various departments of medicine, special reference being very properly made to all native improvements. The proceedings of the Connecticut Medical Society are peculiarly interesting, partly on account of the attractive form in which they appear and partly from the plan which has been adopted by the society to obtain a more general participation on the part of the various counties in the State. This plan, which has lately been inaugurated in Massachusetts, is on its second year's trial in Connecticut. The work was placed in charge of a committee, which issued a circular containing a series of questions relating chiefly to croup and diphtheria. The members of the society were thus enabled to act in unison, and the result is an extensive report embodying a very large experience in the treatment of the diseases in question, in that State. We agree with the committee in thinking that, "with some modifications, which experience can suggest, it will be the most effectual means of any yet proposed of cultivating in the profession in the State a habit of communicating important and valuable information for the general good." The report is illustrated by several handsome plates; there are also one or two quite able original contributions.

The Medical Society of the County of Kings has adopted a novel method in publishing in Brooklyn, New York, a monthly report of their proceedings. It constitutes in fact a monthly medical journal. We see here no reason for refraining from giving some of the innumerable New York journals the benefit of this work, which would thus be more widely disseminated. Indeed, it is to be regretted that the vast amount of work which is annually produced by these state societies should not find a place in our medical journals. The proceedings would appear more promptly, and would obtain a much wider circulation. The system upon which nearly all of our medical journals are conducted is chiefly responsible for this waste of energies. Until this system has been practically revolutionized we cannot expect to obtain a literature which will reflect properly the work of our countrymen and will be read by more than a very limited portion of our professional brethren. Were we fortunate in possessing a few able journals, representing the larger sections of the country but enjoying a national circulation, we venture to assert that American physicians would be startled at the amount and value of the work done in this country, which to a great extent is annually buried beneath the leaves of purely local periodicals.

MEDICAL NOTES.

— The following remarks concerning the health of Rome and Naples, taken from the *Medical Times and Gazette*, will doubtless be read by those of our readers who take an interest in the sanitary arrangements of large cities. The healthiness of Rome is a subject which has been somewhat ventilated in this country lately, and which appears to be now very properly attracting the attention of the medical profession there. The Medical Academy of Rome held an important meeting on the 30th ult., to vindicate the health of the city. Drs. Lanzi and Terrigi read papers on the results of minute analyses of air and other sanitary conditions in the central and suburban quarters, and maintained that Rome, during spring and winter, ranks with the healthiest of Italian cities, and is not only innocuous but beneficial to invalids. The centre of the city remains healthy in summer and autumn, but the suburban quarters, exposed to the Campagna, are then undoubtedly insalubrious. The climate of Rome is gradually improving as the vineyards are built over and drainage extends. Considerable discussion followed the reading of these papers. Dr. Pantaleoni urged that, even in summer, excavating and drainage works did not, as in virgin soils, produce malaria. Dr. Balestra condemned the loose classification of fevers in the weekly health returns. These, if correctly read, would show that the mean mortality did not exceed twenty-four per one thousand. Dr. Ratti exposed the many fallacies in the health returns; *inter alia*, the numerous relapsing cases which appeared as fresh ones when re-admitted to hospital. The debate is to be resumed on the next meeting of the academy. Regarding the sanitary condition of Naples, a correspondent writing from thence on the 23d ult., states, "There is a lamentable insufficiency of good water, the sewerage is as bad as it can be, and the habits of the *bassa classé* are indescribably foul. Still, after all these admissions, travelers have in too many cases to blame themselves for maladies which have been produced by a total disregard of the very different conditions of climate on which they have entered, and the different mode of life imposed upon them. Were proper attention paid to these points there would be no panic about a residence in either Rome or Naples. There is a vast deal to be done to render the sanitary conditions of Naples what they ought to be, and it is satisfactory to state that a deputation of the inhabitants will leave this city to-morrow for Rome, to discuss with the minister of the interior the ways and means of introducing the necessary reforms; but there is surely no little injustice on the part of travelers in holding Southern Italy responsible for what are only the results, in many instances, of their ignorance of the climate, of their extreme imprudence, and violation of the most elementary laws of health."

— In a clinical lecture on cancer of the rectum in the *London Medical Examiner*, Dr. John Marshall remarks: "You may have non-cancerous ulcers of the rectum, which are either strumous, or tuberculous, or dysenteric, or syphilitic, or simple ulcers, from mere want of attention to the condition of the bowels, from neglected piles, or again, in women, from the pressure of an enlarged and displaced uterus upon the bowels. When any of these ulcers are superficial, you can hardly confound them with cancer; but when you meet with a distinct excavation, having somewhat elevated borders, the question will arise

Is this a cancerous ulcer, or is it not? A common ulcer of the rectum is usually situated on the posterior aspect of the bowel, and a syphilitic ulcer quite at the margin of the anus; whilst cancer is usually found higher up, and on any side of the gut. But these distinctions are not absolute. In a doubtful case, from the history and the absence of the true signs of cancer, you may give a provisional opinion, and express a hope that it is a case of non-malignant ulcer, which may be healed. But you cannot be certain; it may be but the beginning of cancer, the symptoms of which may, sooner or later, become evident.

An ulcer of the rectum which is healed, after division of its base and edges with a knife, by cauterization of its surface and edges, by attention to the condition of the bowels and to any special defect in the general health, cannot be cancer; but, on the other hand, if an ulcer goes on from bad to worse, the edges getting thicker and more nodular, and the base more excavated, you are obliged to confess that it is probably, or positively, of a cancerous nature. It is certain, however, that common ulcers of the rectum are not always distinguishable from the earlier conditions of cancer, but they ought always to be distinguished from the later forms of that disease. In the earlier condition you may be sometimes puzzled, and then I advise you to give only a provisional opinion, to treat the case as one of non-malignant ulcer, and to improve the health by giving tonics or the requisite special remedies; let the patient rest, keep the bowels open by injections, and quiet by morphia suppositories, and divide the ulcer, or treat the edges with caustic. Endeavor to get the ulcer to heal, and in many cases you will succeed."

— Dr. A. B. Judson, in a paper read before the New York Academy of Medicine, attempts to explain the cause of rotation in lateral curvation of the spine. The distinguishing feature of the explanation of rotation here proposed is, the recognition of the fact, heretofore overlooked, so far as the author is aware, that the posterior portion of the vertebral column, being a part of the dorsal parietes of the chest and abdomen, is confined in the median plane of the trunk, while the anterior portion of the column, projecting into the thoracic and abdominal cavities, and devoid of lateral attachments, is at liberty to, and physiologically does, move to the right and left of the median plane. Photographs of an ingenious contrivance, by which the spinous processes are held still by horizontal threads while the bodies are unhindered in their movements, show how readily rotation takes place when a slight lateral curve is given to the spine.

— Dr. Dickson, an English navy surgeon at Constantinople, has replied to the insinuations made by the English and French journals and newspapers that the report of the medical commission on the death of the late Sultan was not justified by the facts. From his account it appears evident that the Sultan had been suffering from mania, with paroxysms of fury, and that he undoubtedly died by his own hand.

— The *New York Medical Register* for the ensuing year (Dr. A. E. M. Purdy, editor) is published. It contains the names of 3805 physicians, divided as follows: in New York city and county, 1190; Brooklyn and Kings County, 380; the remainder of New York State, alphabetically arranged, 1267; in New Jersey, 589; and in Connecticut, 379.

BOSTON CITY HOSPITAL.

MEDICAL CASES OF DR. R. T. EDES.

Ovarian Tumor; Repeated Paracenteses; Spontaneous Perforation of Cyst; General Peritonitis; Death. — J. DeL., aged twenty-two, entered the hospital several times at intervals of a few months, for the purpose of obtaining, by paracentesis, relief from her abdominal swelling.

The swelling began about three and a half years ago with some pain in the right iliac region, and extended all over the abdomen. After the first tapping, at which twenty-eight pints of clear syrupy fluid were withdrawn, it refilled in five months, and again, after repeated tappings, at intervals of five, three, three, two and a half, two months, and one month. After some of these tappings a tumor could be felt in the right iliac region, but it is stated that none was felt per vaginam. The fluid, during the latter part of the time, was thick and dirty, sometimes with distinct masses of pus. The patient usually left the hospital in a day or two and returned to her usual avocations, although there was sometimes sufficient pain to require a dose or two of morphia.

On June 28th she was brought to the hospital in a state of collapse, with restlessness, vomiting of coffee-like fluid, and imperceptible pulse.

It was ascertained that since she was discharged, on May 26th, she had been doing housework as usual, when, on the day before she returned to the hospital, she was suddenly seized with abdominal pain and vomiting. She died in a few hours after reëntering.

The autopsy disclosed general peritonitis. On the right side of the median line lay the greater part of a compound ovarian cystic tumor, the largest cyst being adherent to the abdominal walls at the median line, as well as slightly to the omentum. Its walls were from a sixteenth to an eighth of an inch in thickness, roughened on the inside, and contained a somewhat gelatinous fluid, with much pus. At the upper part, and somewhat to the left, was a small perforation, perhaps one eighth of an inch in diameter, which corresponded to a reddened, rough, and ulcerated patch upon the inside. It was about four inches above and to the left of the place where the trocar usually entered the cyst. The other cysts were much smaller and filled with clear gelatinous material.

It is not known whether ovariotomy had ever been proposed to this patient before the last time at which she left the hospital, relieved. It is doubtful if she would have acceded, as the relief from tapping was so complete and her general health was unaffected.

Fibroid of Posterior Lip of Cervix Uteri forming a large Sloughing Vaginal Tumor; Removal by Enucleation; Recovery.[1] — A. J., a German woman aged fifty, married, but not living with her husband for some years, had had severe metrorrhagias in 1875, but considered her health good until four weeks before entrance, when her abdomen began to swell, and she had pain in her back and hypogastrium. Her urine began to dribble away, and she had no appetite. Her urine was very abundant, having been drawn by catheter the day before she entered, and seventy ounces being taken in the same way the day after. It was of specific gravity 1008 to 1010, and contained a trace of albumen, but no sugar. Her appearance was exceedingly anæmic, and a considerable degree of fever was present.

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[1] Reported to the Boston Society for Medical Improvement, May 22, 1876.

The first examination, on May 15th, disclosed the presence in the vagina of a large, hard, sloughing tumor, with an exceedingly offensive discharge. It was apparently posterior to the os uteri, which was represented by a ragged orifice into which the finger could be passed. The sound passed to about the normal distance. The tumor was somewhat smaller above than below, but had no distinct pedicle, and was considered to be a fibroid connected with the posterior lip of the cervix.

On the next day, when preparations were made for the removal of the tumor, a second examination, under ether, with the assistance of Dr. G. H. Lyman and Dr. O. W. Doe, showed the essential correctness of the previous diagnosis, the uterus itself appearing as a mere appendage of the tumor situated upon its upper and anterior portion, and occupying nearly its normal place. The bladder and rectum were also in their natural positions.

A copper wire was placed around the tumor so as to clear the os, and also a sort of flap posterior to the chief mass, thought to be possibly a portion of the normal tissue of the uterus, a supposition which soon turned out to be incorrect. A considerable portion of the hard mass was left above the line of the wire. Upon tightening the wire by the écraseur it soon broke, and this accident was repeated with both the single wire and a wire rope. While we were awaiting an iron wire, which had been sent for, the tumor, which had been dragged down to the vulvar orifice, was incised by a knife, and the usual white structure of a fibroid disclosed. It was then found that it could be separated into lobes and layers by the fingers and knife, without cutting. This process gave us first a large flap from the anterior surface, which had been partly cut off by the wire. Another portion was then dragged out of the vulva, the fingers constantly separating it from its attachments either to the uterus or to the rest of the tumor, but as the patient's pulse began to flag a little, it was cut off by the knife, without any hæmorrhage, with the intention of allowing the patient to come out of the ether and of finishing the removal at another time.

Under the inspiration of Dr. Lyman's enthusiasm, however, aided by his vigorous traction upon the remaining portion, a still larger mass was rapidly enucleated in the same way, the patient having in the mean time received several hypodermic injections of brandy. Several smaller fragments were easily separated, when it was found that nothing but a sort of ragged bunch of comparatively insignificant size remained at the upper part of the vagina. In fact, so closely was it removed and so much did some portions resemble the normal structure of the uterus that it was for a moment feared that a portion of that organ had been removed sufficient to make an opening into the peritoneum. It proved, however, to be hardly disturbed from its normal place. The various portions must have made a tumor equal in size at least to two small fists. They were not immediately weighed, and having been preserved in an aqueous solution of chloral their present weight, one pound five and three quarters ounces, is perhaps a slight exaggeration.

The patient rallied well from the ether. Antiseptic injections of carbolic acid and of chloral were freely used, and quinia administered for a short time, but as it seemed disagreeable to her, and especially as there were never any signs of inflammatory or septicæmic action, it was soon omitted.

On May 21st, three days after the removal of the tumor, a vaginal examination showed a bunch of fragments at the upper part of the vagina resembling swollen piles, among which a sound could be passed into the uterus. A portion of one was cut off with scissors, and the stump touched with strong carbolic acid.

On June 5th the os was found large, soft, not abraded, although bleeding easily when touched. The sound passed less than three inches. The condition of the patient improved until she was discharged, on July 5th, still somewhat anæmic, but otherwise well. The urine continued copious, but there was no longer any trouble in passing it.

The following table shows the range of temperature for a few days.

May.	Morning.		Evening.
16th	100½°		102½°
17th	99¾°		103½°
18th	101¼°	Removal of tumor.	104½°
19th	100¼°		101½°
20th	100¼°		100¼°
21st	99½°	Vaginal examination.	101 °
22d	99½°		100¾°
23d	99¾°		100½°
24th	99 °		100½°
25th	97¾°		98¾°
26th	99½°		101½°
27th	98½°		98½°
28th	98°		99½°

From this point the temperature remained a little above normal, on two occasions reaching 101°. On June 15th it was about normal.

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LETTER FROM BERLIN.

MESSRS. EDITORS, — The death of Louis Traube had not lost its first impression when the medical department of the Berlin University was again summoned to the observance of similar ceremonies by the death of Professor Ehrenberg, at the ripe age of eighty-one years. What the former was to experimental clinical medicine, the latter was to scientific medicine. Both were investigators, and deduced laws under which details arrayed themselves in natural subordination and proof. The two were as dissimilar in their recognition at court, the former having been a martyr to his religious tenets, the latter a pronounced favorite. Ehrenberg turned all this to honorable account in his scientific investigations; Traube was for twenty-four years a clinical leader *without a title,* and shortly before his death rejoiced not only in his personal appointment as professor by the Imperial Minister of Ecclesiastical Education, but also that United Germany chose in him a fit opportunity to illustrate her religious policy.

Christian Gottfried Ehrenberg was born in Delitzsch, April 19, 1795. At the age of twenty years he went to Leipsic to study theology. The inclination of his youth, however, was to medical studies, and he remained but half a year in Leipsic as a theological student. In 1817 he came to Berlin, and matriculated in the young university. To physiological chemistry he devoted most of his time; in 1818, while yet a student, he made a contribution to the systematic study of fungous growths. His inaugural dissertation was an effort in the same direction, its topic being Sylvæ Micologicæ Berolinenses. It displayed independent and bold thought, and added to the physiological knowledge of these Berlin growths; it gave its author the reputation of critic of the idea, then existing, of the transformation of inorganic substances into organic. The dissertation, as is here the custom when the case merits it, was used as authority, and was for sale at the book-stalls under the name and protection of the university.

A more extensive report of his investigations into the development of fungus and mold he communicated in the transactions of the Leopold Academy of Naturalists of Bonn, in 1820. In this same year, under the auspices of the Academy of Natural Sciences, Berlin, he made a scientific tour with Dr. Hempsich, a distinguished naturalist of Berlin. During an absence of six years the Libyan Desert, Middle Egypt, Syria, and Arabia were visited; the report of these researches was published under the title, Natural History Travels through North Africa and West Asia in the Years 1820–1825. Soon after his return he was made extraordinary professor in the Medical Faculty of the University of Berlin. In 1829 he was selected by von Humboldt to accompany him on his researches in Siberia, more particularly in the Ural Mountains. The expedition was fitted out with a leader in nearly every department of natural science, and furnished with money without stint. The Russian emperor prepared relays of horses, halting places, and every possible convenience, so long as the expedition was in his domain. The results of the two years' investigations were published together, those of Ehrenberg appearing side by side with those of the then renowned Humboldt.

Continuing investigation in the department of animal physiology, he published between 1828 and 1834, under the name of Symbolæ Physicæ, contributions to the anatomy and physiology of the lower animals. These were known respectively as Symbolæ Physicæ Mammalium, Avium, Insectorum, and Animalium Evertebratorum. In 1835 he published an article on Phosphorescence, interesting from a scientific stand-point, together with a review of the chronological development of this remarkable phenomenon, which he attributed to infusoria. Three years later, Infusoria as Perfect Organisms, a Glance at the Deeper Life of Organic Nature, was published by him. This work was not only rich in new materials, but contained a systematic review of all that was heretofore known of infusoria and radiata. Though devoted to a specialty from the character of the investigation, it was so comprehensive in its range as to win the consideration and position of a text-book. In 1839 he was promoted to an ordinary professorship. This year found him elected foreign member of several scientific societies, of which the Royal Society of England paid him the most distinguished honor. Among other distinctions given him at this time of his life was his election, in 1842, as one of the thirty knights of the order of the Friedens Klasse, accompanied by the decoration *pour la mérite* for science and art. One of his last contributions to scientific literature was the voluminous work on mineralogical formations by microscopic organisms under the title of Micro-Geology.

The decadence of Ehrenberg's life was marked by a series of personal griefs, the chief of which were the death of his wife and the gradual loss of sight by cataract. He lived but a few weeks after the successful removal of the cataract, and died in the infirmity of old age. His burial was imposing in the style of its celebration, and honored by the character of the personages in attendance. Nothing, perhaps, can lend to the popular eye more *stateliness of the state* in a government whose head has royal blood than, on the occasion of the death of a citizen eminent at once as a scholar and as a court favorite, to honor the sad solemnity by its manifest recognition. The funeral cortege was headed by the "gala-equipage" of the emperor, and passed through a thickly-settled portion of the town out to the Marienkirchhof, near the Prenzlauer Thor. Among the nearest mourners were the elder professors of the medical, natural science, and natural history faculties. To a republican and non-royalist it may not be an unfair question to consider, which will outlive the other; whether the scientific achievements of the scholar, in the thinking, critical mind of his natural audience, or the imposing influence of royal recognition at burial, upon the public eye. The reader must put himself in Prussia, certainly out of America, else the suggestion is devoid of meaning.

With such allusion to his death and burial, it is proper to introduce the residence of Ehrenberg. It is a two-story house in Franzoesische Strasse, on which is an inscription in the classical German of that day, indicating that it was devoted by Friedrich Wilhelm the Second, to the Society of Natural Philosophy, which was founded in 1773, and was the oldest scientific body in Berlin. In the prime of life of its late possessor, it was the repository of his private collections from the natural history of Egypt, Siberia, and Arabia. Not only the personal attractions of Ehrenberg, but the public interest of its contents, gave the house a peculiar charm. MED.

BERLIN, PRUSSIA, *July* 15, 1876.

LETTER FROM DR. DALTON.

MESSRS. EDITORS, — My attention has been called to a paragraph in the *Boston Herald* of August 3d, purporting to be a report of the testimony of Dr. J. B. Treadwell at the inquest in the case of Clara T. Fisher, as follows: —

"Dr. Treadwell was recalled, and testified that Prof. John C. Dalton, of New York, examined the uterus and the right ovary of the late Clara T. Fisher, in Dr. Treadwell's office, August 1st. He said there was unmistakable evidence of pregnancy, which had existed about two months."

The statement, as quoted above, might give the impression that I had been able to say, from my own independent inspection of the uterus and ovary, that pregnancy had existed in this case, and that I fixed its date with some precision. This impression, however, would not be entirely correct. The ovary, when I examined it, contained a corpus luteum, well developed and of normal structure, showing a colorless central clot and a convoluted wall of a rather dull yellow. It is well known that two of the most important elements in fixing the true character of a corpus luteum, apart from its volume and structure, are the colors of its central clot and of its convoluted wall. But at the time of my examination the specimens were nine days old, and had been kept in a preservative fluid, which might have altered the color of either or both its parts. Dr. Treadwell informed me that at the time of the autopsy the central clot was also perfectly colorless, and that the convoluted wall was of a bright chrome yellow. I have no reason to doubt that his observations were correctly made, and consequently that this was a true corpus luteum of pregnancy; but I was necessarily guided, in forming this opinion, by his report of the condition of the parts when fresh. I stated my opinion unreservedly to Dr. Treadwell, without calling his attention, in so many words, to the specific grounds upon which it was based; judging, from his evident familiarity with the subject, that he appreciated fully the importance and bearing of my previous questions and his replies. This does not imply any censure upon Dr. Treadwell for the manner in which his testimony was rendered at the inquest; but since my opinion has been quoted as evidence, I think it proper to say exactly how it was formed.

With regard to the uterus, I could not form a distinct judgment from the appearances visible at that time. The inner surface of the organ, at the points where the sections had been made, was softened and ragged. Dr. Treadwell, I understand, had previously found fragments of the chorion entangled with it. The presence of these bodies would of course render the fact of pregnancy absolutely certain. I did not find them at the time of my examination. But this is no evidence that they had not existed when the organ was fresh, as they might readily have been detached by subsequent manipulation and the contact of the preservative fluid. Yours very respectfully, J. C. Dalton.

Boston, *August* 10, 1876.

LETTER FROM DR. TREADWELL.

Messrs. Editors, — Prof. J. C. Dalton has kindly shown me a communication which he intends to publish in your Journal. I wish to say that at the request of Dr. A. D. Sinclair I gladly submitted the organs in question to Professor Dalton for examination. After this examination had been made, the coroner, having become aware of the fact through a letter written to him by Dr. Sinclair, recalled me before his jury and asked me to state whether or not Professor Dalton had examined the organs in question, and, if he had, to state what he said as to the evidence of the preëxistence of pregnancy. I replied that Professor Dalton had examined the right ovary and portions of the uterus, and expressed himself as confident that the woman had been pregnant.

At the time of his examination Professor Dalton made the general statement that he had no doubt the woman had been pregnant, and that he drew his conclusions from the appearance of the corpus luteum; and, as he says in his letter, did *not* specify the particular points which enabled him to form his opinion; these I had no means of knowing, — inference not being knowledge, — and consequently in what I said before the jury I was obliged to confine myself to what I saw Dr. Dalton do, and his bare statement that in his opinion pregnancy had existed.

In alluding to the question of pregnancy in the Fisher case the following language is used in your editorial of last week: "The fact that they" — the jury — "were willing to receive evidence bearing on a question," etc. The phrase "were willing to receive" would seem to imply that evidence upon the point in question was volunteered by some one, and if by anybody it must have been by me. Such was not the case, for having undertaken to make the post-mortem examination, I was bound by fealty to the commonwealth, the medical profession, and my own conscience to make it a thorough one; and having made it, I was bound by my oath as a witness to tell all that I found, which I did in my written report without giving any opinions. It would have been manifestly absurd and criminal for me to have withheld, as it has been claimed I should have done, that portion of it relating to the organs of generation. This is all wrong. If I found the uterus in an abnormal condition I was just as much bound to tell of it as I was to say that the heart was unsound, if such were the case. Had I withheld a portion of my testimony from the jury, on the ground that in my opinion it did not have any bearing on the death of the woman, I should by so doing have constituted myself jury as well as pathologist and witness.

The question of relevancy was one for the jury, and the jury could not determine the relevancy or irrelevancy of the point in question until they had heard the evidence. To contend that they could have done so would be equivalent to saying that they could have given a fair and truthful verdict without any investigation.

The knowledge of the fact of pregnancy is sometimes of the greatest importance to the officers of the law, in conducting a criminal case, and the surgeon who makes the post-mortem examination cannot tell what the course and termination of the case may be, nor what may be required of him as a witness in connection with it. One case should be the same to him as another, and in every case he is bound to get all the information he can from the autopsy. Of course as a witness he is bound like all other witnesses to tell the truth in regard to whatever he may be inquired of, no matter whether it appears to him pertinent or not. As to what is pertinent the tribunal before which he testifies must decide, and not he nor the public.

The JOURNAL represents me as saying that the wall of the corpus luteum was "congested;" "corrugated" was the word I used. The appearances represented by these two words would give diametrically opposite indications in regard to the real character of the corpus luteum.

In regard to the doubt expressed as to whether in every case of pregnancy a true corpus luteum is formed, I must say that among all of the cases which I have seen reported as confirmatory of this doubt, I have not been able to find one in which there is proof that pregnancy had existed for the length of time ordinarily sufficient to produce the characteristic appearances of the true corpus luteum, in which there is not something and sufficient to show that wrong conclusions have been drawn from it.

The clause "in the present state of knowledge" (1844), in the paragraph quoted from Wharton Jones, is quite significant as to how much weight should be given to his opinion now.

It *is* quite true "that even prior to 1850" the question was raised as to whether the corpus luteum of pregnancy could be distinguished from that of menstruation; but it is still more true, it seems to me, that nearly all the doubt that has obtained in regard to this point existed previous to that time.

It seems a little strange, in view of what we know at the present time in respect to the appearance of these bodies, to quote a statement made in 1842, that all corpora lutea "have the same form and the same structure."

The *opinions* of authors who wrote previous to 1847, or thereabouts, are to be regarded as of little weight at the present time, from the fact that until the results of the researches of Bischoff, Coste, and Dalton — I name these authors in chronological order — had been made public, no real scientific knowledge had been attained, upon which any opinions could be based. Coste was rather in advance of Dalton in point of time, but Dalton worked out his conclusions in ignorance of the results of the investigations of Coste, and they agree substantially in their conclusions. Nearly all of our present knowledge of the subject is the result of their investigations, together with that of Bischoff, and not only have their conclusions never been refuted, but subsequent observation has, I think I may safely say, confirmed them.

One great source of fallacy in the conclusions of authors who wrote prior to 1847–50 arises from the fact that they were not aware of, or did not give sufficient weight to, the fact that during the first three or four weeks of pregnancy there is no appreciable difference between the corpus luteum of pregnancy and that of simple menstruation.

Bischoff is quoted as disputing the diagnostic value of the so-called *true* corpus luteum in 1843. About 1853 or 1854, I think, although I have not been able to obtain a copy of the periodical in which the article was originally published, he wrote as follows: "The corpus luteum of conception, on the other hand, goes on developing itself throughout the first months of pregnancy, and attains a fullness of size, color, and texture which the menstrual corpora lutea never reach. It lasts throughout the whole period of pregnancy, and disappears after delivery."

Writers upon this subject may be divided into two classes: those that have investigated the matter thoroughly and *worked* out their own conclusions, and those who have simply formed their opinions from the writings and opinions of others. I think it is a fact that those of the first class named, almost without exception, hold to the diagnostic value of the corpus luteum.

Want of space forbids my even giving a list of the host of authors who hold the opinion that the so-called true corpus luteum is really and unmistakably an indication of pregnancy.

In regard to the case reported by Dr. Benham I have only a word to say, and that is this: given, a uterus with its interior lined with such a decidua as Dr. Benham describes, and an enlarged ovum covered with a shaggy coat, which, judging from the detailed description given of it, could be nothing but the shaggy coat of the chorion, there is no doubt in my mind that conception had taken place, and that the corpus luteum in question was one of pregnancy. The seeming discrepancy between the age of the ovum and that of the corpus luteum may be readily accounted for by either of two hypotheses. Against the evidence afforded by these appearances, the bare statement that the conditions surrounding the woman were such that she could not have had "sexual connection with any one *for at least several years before*," supplemented by the assertion "*that externally the organs of generation presented in a marked degree all of the highly characteristic signs of virginity*." (Italics are mine.) I know of no more "highly characteristic sign of virginity" than a perfect and extensive hymen, and yet women have come to childbed with that sign of virginity in all of its perfection. That Dr. Benham is inclined to be hasty in his conclusions will, I think, appear from the fact that he says he thinks "that this individual case goes a long way to make it probable" that the ovum is extruded from the ovary more frequently at the beginning of menstruation than at any other time; while on the opposite page he gives a case in which menstruation had been going on three or four days and no ovum had at that time been discharged, although one Graafian vesicle seemed about ready to burst.

In conclusion, I wish to say that the question of pregnancy in the Fisher case was not settled in my mind by the evidence derived from the corpus luteum alone, but that this evidence was regarded as simply corroborative of other appearances which in themselves gave conclusive proof of the fact.

Very respectfully yours, J. B. TREADWELL.

BOSTON, *August* 12, 1876.

[In the editorial referred to, no allusion was made to Dr. Treadwell's evidence beyond his testimony in regard to the significance of the corpus luteum. — EDS.]

MEDICAL DEPARTMENT OF THE UNIVERSITY OF THE CITY OF NEW YORK.

IN relation to the remarks made by our correspondent in the JOURNAL of July 20th, in regard to the university, we understand officially that —

(1.) Professor Budd will continue to hold his clinics in that institution as usual. Dr. Gouley takes Dr. Mason's place in the university division of Bellevue Hospital, and gives clinics on his specialty, so that the number of clinics will be greater than has ever been given by professors in the university during any previous winter session.

(2.) As regards the financial condition of the university, information can only be obtained through the council, but the medical faculty are not aware that any financial embarrassment exists in any of the departments.

(3.) Neither the council nor the governing faculty have ever made promises or had any implied understanding in regard to positions, with gentlemen holding subordinate places in the medical department of the university.

(4.) The changes which the faculty of the medical department of the university recently made had for their object the improvement of the teaching in the college.

COMPARATIVE MORTALITY-RATES FOR THE WEEK ENDING AUGUST 5, 1876.

	Estimated Population.	Total Mortality for the Week.	Annual Death-Rate per 1000 during Week.
New York	1,060,000	609	29.88
Philadelphia	825,594	415	26.14
Brooklyn	506,223	255	26.19
Chicago	420,000	266	32.93
Boston	360,000	191	27.59
Providence	101,000	38	19.62
Worcester	51,300	26	26.35
Lowell	51,700	33	33.19
Cambridge	50,000	21	21.84
Fall River	47,200	36	39.66
Lawrence	36,000	13	18.72
Lynn	34,000	22	33.64
Springfield	31,400	14	23.18
Salem	26,500	19	37.28

Normal Death-Rate, 17 per 1000.

MESSRS. EDITORS, — I cannot forbear saying a few words in regard to the following case, which was reported in your issue of the 27th ult. : —

"Protracted Jaundice, with Severe Constitutional Symptoms, successfully Treated by Choleate of Soda." The case was confessedly one of malarial origin. "Twelve years ago had intermittent fever." "For the past four or five months has had chills and fever at intervals of two or three weeks." "May 29th. Had a chill of two hours' duration, followed by a fever of three hours." "May 31st. Severe chill this morning." "June 2d. Chill three hours, followed by fever." Thus markedly *tertian* in character. "June 2d. Pil. quiniæ sulph. gr. x. to night." "June 3d. Pil. quiniæ sulph. every night, gr. iv." "June 4th. No chill, but a slight amount of fever, this morning."

Thus, after the use of heroic doses of quinia the chills cease and convalescence is established. "June 4th. No chill. Pil. sodæ choleat. gr. vi. every night." After a month of experimenting this agent was first employed, and "its use in this case was in some sense empirical, being based upon its known power as a solvent of gall-stones."

The history as given shows a want of care in making the diagnosis; this was followed by an empirical treatment, and at the end credit was withheld from an invaluable agent with already established reputation as almost a specific in such cases, and credit and prominence given to one scarcely known to the profession. Yours respectfully, H. R. R.

DIED. — At Brighton, 31st ult., Dr. Isaac G. Braman, aged sixty-three.

MILITARY APPOINTMENTS IN THE MEDICAL STAFF, M. V. M. — July 26, 1876, Fifth Regiment of Infantry, Surgeon Edward J. Forster, of Boston, to date from May 19, 1871. Assistant Surgeon Horace E. Marion, of Boston, to date from April 13, 1875.

Eighth Regiment of Infantry, Surgeon Preston M. Chase, of Danvers, to date from January 13, 1875. Assistant Surgeon Charles A. Carlton, of Salem, to date from March 5, 1875.

August 10th, 1876, Fourth Battalion of Infantry, Surgeon J. L. Hildreth, of Cambridge, to date from March 29, 1875. The foregoing are re-appointments to former positions.

August 7, 1876, First Battalion of Artillery, Surgeon David Dana, of Lawrence. Dr. Dana was surgeon of the late Second Battalion of Artillery.

August 12, 1876, Sixth Regiment of Infantry, Surgeon George E. Pinkham, of Lowell. Before the reorganization of the militia Dr. Pinkham was the assistant surgeon of the regiment.

CORONER APPOINTED. — Joshua B. Treadwell, M. D.